

material in a region at that time thousands of miles from any other embryologist or morphological zoologist. The work could be done nowhere else, but try as I might, not a cent could I secure from anywhere to support me in my work.<sup>1</sup> The result of that work was, aside from smaller papers describing new species, etc.:

1. 'On the Precocious Segregation of the Sex Cells in *Micrometrus aggregatus* Gibbons,' *Journ. Morph.*, V., pp. 480-492, 1 plate.

2. 'The Fishes of San Diego,' *Proc. U. S. Nat. Mus.*, XV., pp. 123-178, 9 plates. Giving spawning seasons and embryology, as well as a list of San Diego fishes.

3. 'On the Viviparous Fishes of the Pacific Coast of North America,' *Bull. U. S. Fish Comm.* for 1892, pp. 381-478, 27 plates.

4. 'Sex-differentiation in the Viviparous Teleost *Cymatogaster*,' *Arch. f. Entwickl.-ungsm.*, IV., pp. 125-179, 6 plates.

A request for fifty cents a day while working in California was declined by one of the government bureaus. A request for assistance from two other institutions was declined.

The article 'On the Viviparous Fishes' was sold for \$100, not quite twice as much as had been paid to draw one of the figures submitted to illustrate it. It is thus that scientific work has been encouraged in the past. The article was a fragment, and the viviparous fishes still await a worker *who must be in the 'field'* among those fishes. Wallace long ago pointed out that individual workers in the field do proportionately vastly more than big, expensive government expeditions. Just as surely vastly more will be accomplished if individual workers are subsidized to do their work where they can do it best than if they are herded at Washington.

The most urgent need is temporary or permanent research professorships: appointments made for specific work of men who will receive their pay from the appointing institution, who are responsible for all of their time and results to the appointing institution, but who carry on their work in their home institution or

<sup>1</sup>Thirty odd dollars I spent in incidentally picking up rare or new species of fishes were refunded later.

wherever else their work can be done to best advantage.  
CARL H. EIGENMANN.

#### THE MUTATION THEORY IN ANIMAL EVOLUTION.

THE question of the origin of species is that of the origin of specific characteristics or differential marks. According to one theory they arise gradually by accumulations of the order of fluctuations. According to the other they arise suddenly and completely as mutations. The former theory explains cases in which species are connected by intergrades. The latter best explains discontinuity in species; without it a subsidiary hypothesis to account for observed discontinuity is necessary.

The first reading of de Vries's great work 'Die Mutationstheorie' carried a conviction to the minds of many zoologists as well as botanists that the truth of the discontinuity theory—which has long been urged under other names—had been insufficiently recognized. Of late opposition is appearing and the mutationist is led to reexamine the grounds of his faith. One of the most vigorous of the reactionists is Merriam<sup>1</sup> (1906), who concludes his address before the zoological section of the American Association with the words: "The theory of the origin of species by mutation, therefore, far from being a great principle in biology, as some seem to believe, appears to be one of a hundred minor factors to be considered in rare cases as a possible explanation of the origin of particular species of plants, but, so far as known, not applicable in the case of animals."

The evidence for so sweeping a generalization is to be looked for in the body of the address and I have carefully reread and analyzed his paper. He offers first certain general objections to the mutation theory and then cites cases supporting the alternative theory of gradual modification. His general objections do not seem to me to be important. His query (p. 242) 'if sport variations are less likely to disappear by reversion than are

<sup>1</sup> 'Is Mutation a Factor in the Evolution of the Higher Vertebrates?' *SCIENCE*, XXIII., No. 581, February 16, 1906.

individual variations' would not have been asked if he had bred sports and observed their resistance to blending and reversion. In poultry, tailless birds bred to tailed birds produce in the second generation and later a large proportion of wholly tailless offspring. Crest, frizzled feathers, tendency to produce pigment in the connective tissue, dominate over the normal conditions. When birds with a down-like modification of the adult plumage are bred together all of the offspring have that peculiarity. Even the polydactile condition does not blend with the normal. After these facts what becomes of the 'opinion' that has been bandied about for over a generation and is resuscitated by Merriam that sports are lost by swamping? Regrettable is Merriam's innuendo directed toward zoologists who have been trained in analytical methods involving the use of the microscope. I am not convinced that analytical training in the laboratory is a less adequate training for tackling the species problem than setting traps and shooting and skinning mammals and birds in the field. What the problem demands is an analysis of species into their constituent *characteristics*, a study of the behavior of these characteristics in the field and the laboratory under controlled environmental conditions and a study of their inheritance.

Certain special cases are cited at some length by Merriam as disproving the mutation theory. The number might have been greatly increased. In general it may be said the widely ranging species of small mammals and non-migratory birds in North America exhibit remarkable parallel changes in coloration, gaining a darker and brighter pigmentation as one passes from the dry plains or the interior deserts to the moister Pacific coast from northern California to Alaska. It is hardly conceivable that mutations of exactly the same sort should affect so many species in the same way. It seems more reasonable to ascribe these changes to climatic conditions. Whether these changes are permanent enough to warrant calling them specific characteristics is uncertain. Mr. Chapman tells me that there is evidence that in the case of certain species originating in the interior,

one section spread to the southern deserts, where it became pale, while another section spread northward, where it was rendered dark, and that these two sections have subsequently approached each other on the Pacific coast, where they are strikingly dissimilar, although near together. Such a history, which deserves working out in detail, would indicate a persistence of the climatic modifications. I, for one, am quite satisfied that geographic variation, determined directly or indirectly by climate, is not of the order of mutation and may involve a permanent modification of the germ-plasm. The variation is often sufficient to justify assignment of the extremes to distinct species whenever intergrades are missing. These geographic variations are, however, it must be confessed, largely of a quantitative rather than of a qualitative sort; *i. e.*, usually new characters are not involved, but only a modification of characters, as, *e. g.*, in the case of mammals an increase of the black and red pigment. It may well be that species founded on quantitative differences follow a different method of evolution than those founded on qualitative ones. I, for one, while an adherent of the mutation theory, still maintain the view expressed by me in 1904,<sup>2</sup> when I used some of the same kind of evidence employed by Merriam. This view is that the process of evolution is complex enough to admit of many 'factors' and evolution is not always by mutation.

The real argument for discontinuity in evolution is the occurrence of characteristics in nature that are discontinuous and which never show intergrades. The mere fact of discontinuity between species of the same genus is not sufficient to prove that they have arisen by mutation. It must be shown that the differential characters are in essence discontinuous. The practical way to get at the true nature of characteristics, whether continuous or discontinuous, is by their behavior in inheritance. If, in cross-breeding, a character tends to blend with the dissimilar character of its consort it must be concluded that the character can be fractionized and that

<sup>2</sup> 'Evolution without Mutation,' *Journal of Experimental Zoology*, II., pp. 137-143.

intergrades are possible. If, on the contrary, the characteristic refuses to blend, but comes out of the cross intact, as it went in, the conclusion seems justified that the characteristic is essentially integral and must have arisen completely formed, and hence discontinuously.

Using this criterion, I have of late been testing the application of the mutation theory to animals and have had an opportunity to examine the experiments of others. Some of the work has been done on the characteristics of domesticated 'races,' others on wild varieties. There seems to be no difference in the behavior of characteristics of domesticated and wild varieties. The result is that most characteristics, but not all, fail to blend and are strictly alternative in inheritance. I interpret this to mean that the characteristic depends on a certain molecular condition that does not fractionize. The inference is that if the characteristic is incapable of gradations now it has always been and hence must have arisen without gradations, *i. e.*, discontinuously. Examples of such discontinuous characteristics are the spots on the elytra of certain beetles, the crest on the canary, the form of the comb in poultry, extra toes, black plumage and color of iris. One who sees the striking failure of these characteristics and many others to be modified in any important way will feel convinced that they are not capable of forming intergrades and hence could not have arisen gradually.

While I am not of those who would seem to deny that characters of domesticated species are as *natural* as any others, it is worth inquiring whether discontinuous variations, such as I have been dealing with, occur among feral animals. The evidence is that they do. Thus our gray squirrels exhibit in many localities a striking number of black individuals. These are not found everywhere, but in small areas may be fairly common. Difference in climatic conditions can not account for the blacks—they belong to the order of melanic sports, *i. e.*, mutations. Our red squirrels and various other feral rodents sport in the same way. Birds also show melanic sports, *e. g.*, in the European snipe (*Scolopax*

*gallinago*) a chocolate brown form sometimes appears which, like the black squirrel, has been considered by some as a distinct species. Similarly, more or less albinic sports occur in nature. White crows and blackbirds are well known and many individuals of the house sparrow are partially albinic. The history of the twisted beak of the crossbill (*Loxia curvirostra*) is, of course, unknown. The characteristic is, however, the same as, and has probably had a similar origin with, that which suddenly appears in one per cent. of the poultry that I breed and which has been observed as a sport in crows. Scarcely one of the characteristics of poultry may not be found appertaining to some feral species, and there is every reason to believe that these characteristics have the same property of indivisibility in the latter case as in the former. Such facts as I have cited above could be added to by Dr. Merriam or any other naturalist with a similarly extensive and profound knowledge of the higher vertebrates; and they seem to me to lead to the conclusion that some new characters may arise in nature suddenly, as sports or mutations, and persist as specific characteristics.

CHAS. B. DAVENPORT.

STATION FOR EXPERIMENTAL EVOLUTION,  
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#### THE RIGIDITY OF THE EARTH.

TO THE EDITOR OF SCIENCE: In SCIENCE of September 28 Professor L. M. Hoskins is led to the sad conclusion that I have misunderstood Lord Kelvin's definition of the modulus of rigidity, and he thus apparently questions the results which I have given in *Astronomische Nachrichten*, No. 4,104. Owing to the great length of that paper, my explanation of the connection between rigidity as experimentally determined for solid bodies here upon the earth's surface and other bodies kept rigid by pressure was not sufficiently developed; and as the difficulty that has misled Professor Hoskins appears to have occurred also to others, it seems worth while to point out the omitted steps in the chain of reasoning, which will, I think, make it clear that *my process* has been misunderstood and misinterpreted,